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to decide as to the nebulous condition of an object of this kind when at the limit of visibility.

r Tauri was about as bright as the star A of my former paper, and seemed to be involved in a small hazy nebulosity; but the definite nebula in which it shone in 1890 did not exist.

These observations have been made under the best conditions, with the objects on the meridian, upon the following dates :—

1895 September 15, 22, and 23.

Mount Hamilton, California:
1895 *September* 24.

Note on some Remarks of Mons. O. Callandreau in the "Bulletin Astronomique" for November 1895. By E. J. Stone, M.A., F.R.S., Radcliffe Observer.

In the *Bulletin Astronomique* for November 1895, p. 458, there are some remarks by Mons. O. Callandreau, which bear upon my papers on time measurement. But as my papers on this subject have nearly all been printed in the *Monthly Notices*, it is, perhaps, desirable that the few words which I consider necessary in the way of reply should also appear in these publications.

M. Callandreau states, with perfect accuracy, that I base my measures of t, fundamentally, upon the Sun's mean longitude. But he expresses himself as follows :—

"On voit dans l'article dont il s'agit que M. Stone, rompant avec l'habitude des astronomes de mesurer le temps par l'angle horaire du Soleil, procède comme si la longitude moyenne du Soleil devait servir à la mesure du temps.

"Les deux manières sont également admissibles à un point de vue abstrait."

I have given this statement in M. Callandreau's own words on account of its importance.

I hardly think, however, that this statement, *as it will generally be understood*, can be accepted as correct.

If astronomers did, as a matter of fact, consistently and exclusively use a system of time-measures, t', which had no connection whatever with the system of time-measures, t, based on the Sun's mean motion in longitude, which I employ, then it might be possible to admit that my views were mathematically sound, but of no practical importance in the existing state of astronomy. But I have proved, or profess to have proved—whether rightly or wrongly may now be taken as the real point at issue—that astronomers *use in their mathematical investigations* measures of time, t, based directly on the Sun's mean motion in longitude; and, if there is any change of usage, which I assert to be the case, it is they who break away from the fundamental

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system of time-measures, t, in terms of which their mathematical formulæ are expressed; when, on account of the practical difficulties in finding the variable, t, directly from the observations of the Sun, they attempt to determine it from the motion of the meridians in right ascension, or from the relative motions of the meridians and of the Sun in right ascension. Such methods of finding the variable, t, are things much to be desired. And they would be practically successful, for all the values of n' which could be adopted in our theoretical investigations, if the rotation of the Earth on its axis was, as is supposed, constant and the exact *ratio* of the mean motion of a meridian to that of the Sun in R.A. was known. But such is not the case. And the required variable, t, cannot, therefore, be *accurately found* in either of these ways. I believe this is now generally acknowledged.

Approximate values of t can be and are thus found by the use of approximate values of the *ratio* already referred to. But in estimating the errors made by the process adopted, it is necessary to take the error of the adopted value of the constant *ratio* into consideration.

This is what I have done in all my formulæ; and this is what astronomers have hitherto neglected to do. But the terms which I have added to the expression of the R.A. of a meridian as a function of t are certainly required if the measure of the time, t, is based on the Sun's mean longitude. If such had not been the case my formula would have been proved to be erroneous long before this.

To disprove the practical bearing of my views, it is necessary, therefore, that someone should show that astronomers do not establish a connection between the unit in terms of which t measures the time in their mathematical expressions, and n', *the Sun's mean motion in longitude in the unit of time*, when they eliminate from the differential equations of motion the accelerating effects of the mass of the Sun, m', or of the sum of the masses of the Sun and of the Earth, $m'+\mathrm{E}$, or the sums of the masses of the Sun, Earth, and Moon, $m'+\mathrm{E}+m$, by conditional equations such as

$$m' = a'^3 n'^2$$

or

$$m' + \mathrm{E} = \mathrm{A}'^3 n'^2$$

or

$$m' + \mathrm{E} + m = \mathrm{A}''^3 n'^2\,;$$

assume the ratios of the accelerating effects of all the masses to that of the Sun to be constants independent of the units of length and of time, a condition which requires the same units of length and of time to be adopted in all the mathematical expressions; and find the connection between the linear constants a', or A', or A'', and the Sun's distance r', and those between the corresponding results of observation and the other linear and

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angular quantities which appear in their mathematical work subject to one of the conditions mentioned.

I feel some confidence from the serious consideration which I have given to the question, that not only cannot this be shown, but that I have clearly shown the converse to be true. These proofs can be made absolutely rigorous, but they necessarily become somewhat long and formal if every elementary step has to be given.

Note on Newcomb's Tables of the Sun. By A. M. W. Downing, M.A., D.Sc.

Remarking on the large annual term exhibited in the comparison of Le Verrier's and Newcomb's Solar Tables, printed in the supplementary number of the *Monthly Notices* (vol. lv.), Professor Newcomb has pointed out to me that the definitive correction to Le Verrier's value of the eccentricity of the Earth's orbit for 1900, as found in his *Elements and Astronomical Constants* and adopted in the Solar Tables, is +0″·20. The effective correction to Le Verrier's longitude of perigee for the same epoch is +6″·3.

Calling these quantities δe and $\delta\pi$ respectively, we have the corresponding correction to the longitude :

$$\delta l = \frac{dl}{de} \,.\, \delta e + \frac{dl}{e d\pi} \,.\, e\delta\pi.$$

But

$$\frac{dl}{de} = \quad 2 \sin g$$

$$\frac{dl}{e d\pi} = -2 \cos g$$

where g is the mean anomaly.

Therefore,

$$\delta l = +0''\!\cdot\!40 \sin g - 0''\!\cdot\!21 \cos g.$$

Applying this correction to the comparison referred to above, we have (taking the mean of each three consecutive corrections for convenience) :—

Date 1900.	Residual Correction.	Date 1900.	Residual Correction
Jan. 9	−0″·27	July 20	−0″·53
Feb. 2	−0·25	Aug. 13	−0·62
26	−0·26	Sept. 6	−0·57
Mar. 22	−0·33	30	−0·60
Apr. 15	−0·42	Oct. 24	−0·66
May 9	−0·42	Nov. 17	−0·73
June 2	−0·40	Dec. 11	−0·65
26	−0·46	27	−0·73